Section of the History of Medicine

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Richard Caton (1842–1926) Pioneer Electrophysiologist

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RICHARD CATON was one of Liverpool's most distinguished citizens, and when he died in 1926, at the age of 84, the medical profession lost one of its great men.

He graduated from Edinburgh in 1867, and three years later became M.D. and received the gold medal for his thesis on the "Migration of Leucocytes". A year after graduation he settled in Liverpool (he belonged to a Lancashire family originating in Heysham and Caton) and soon became associated with the Liverpool Royal Infirmary School of Medicine as physician to the hospital and lecturer in physiology. He played an important role in the development of that Medical School into the University College of Liverpool, and he was from 1882 to 1891 the first holder of the Chair of Physiology, a post which he combined with that of physician at the Royal Infirmary. It was at that time customary in most medical schools for chairs in anatomy and physiology to be part-time, and held by a surgeon and physician respectively. Caton had long recognised that such joint appointments were unjust and injurious to the preclinical sciences. Within a few years of his arrival in Liverpool, when he was, at the age of 31, Lecturer in Physiology at the Royal Infirmary Medical School, he delivered the introductory annual address on "Physiology in Relation to the Health of the Community and the Advance of Medicine", which reveals an enviable prescience. He pleaded for Government support for medical research in phrases with a singularly modern ring:

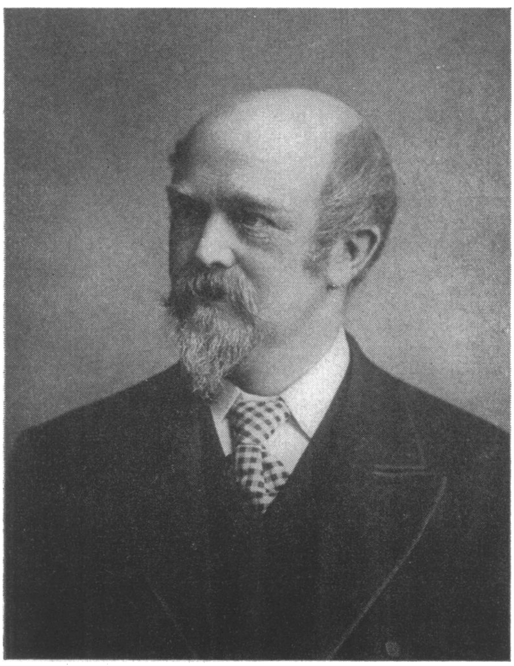

FIG. 1.—Richard Caton.

> "The time will doubtless come when our Government will recognise the importance of aiding this department of science. When we consider the scores of thousands of pounds expended on scientific experiments for the perfecting of weapons, for the discovery of the best mode of defence and attack in warfare, to say nothing of the millions spent in carrying out such results, the ultimate object of which, of course, is the defence and preservation of national life and property; when we consider the vast sums thus spent, it would not appear unreasonable to expend a few thousands, a mere fraction in comparison, in aid of the investigation of other modes of saving life—life which is in much greater actual peril in another way. There are other foes besides military invaders."

Sir Charles Sherrington and Sir Henry Dale have both recalled how in their early days distinguished physicians poured scorn on physiological experiment as an aid to the interpretation of the phenomena of disease. Yet here in 1873 is Caton's exhortation:

> "Let me recommend you to pay attention to each department of study; don't undervalue parts which at present may seem to you to possess no

immediate utility in Medical practice, such as branches of chemistry or practical physiology. You will subsequently find how valuable they are. Scientific knowledge is becoming more and more important to the Medical man. During the period in which you will be practising your profession, Medicine will advance rapidly. If we could look forward twenty years hence, we should probably find that some astonishing strides had been made. Let me, then, strongly urge upon you, while giving a due attention to practical Medicine and Surgery, to take care that you acquire a sufficiently sound basis of scientific knowledge to be able to keep pace with the advances of your profession."

And one final quotation from this address which embodies his plea for a full-time chair of physiology:

> "The work of investigation, as I have said, is difficult, laborious, and often expensive. At present it is carried on mainly by medical practitioners, who earn their bread by the practice of their profession, and who gain little or nothing by their scientific work, except the esteem of the few who are able to understand and estimate it. The public in general know nothing about these subjects. The science is thus at a disadvantage, depending too much on the efforts of isolated men who can only spare to it a small part of their time. The gain to the science, and the ultimate benefit to the country, would probably be great if one or more Government-supported laboratories were established, and some of the more eminent physiologists paid to devote their entire time to the work."

By 1891, he had persuaded George Holt, a member of a family of Liverpool ship owners, whose name is identified with many of Liverpool's philanthropic enterprises, to endow a full-time chair in the University College. Its first incumbent was Francis Gotch, who left four years later to become the first Waynflete Professor of Physiology in the University of Oxford. He was succeeded by Sherrington who held the George Holt Chair for eighteen years, during which many of his classical contributions to the physiology of the nervous system were made.

Caton's interest in the University he did so much to establish never waned. He reached the high office of Pro-Chancellor, and was its first representative on the General Medical Council, and held that office until he died in 1926.

He became a city councillor and devoted much time to the promotion of higher standards of public health. In 1907, to the gratification of all, he was chosen Lord Mayor of the city. He was a classicist and historian. He travelled extensively in the eastern Mediterranean countries, and published papers on "The Temples and Ritual of Asklepios, Hippocrates and Cos", and "The Medicine and Medicine God of the Egyptians." He served on the council of the Royal College of Physicians and delivered the Harveian Oration. During the First World War he was honorary Colonel, West Lancashire Division, R.A.M.C., and was tireless in his efforts to secure the comfort and nursing of the sick. He was in so many good causes a selfless and indefatigable worker, and contributed felicitously to so many humane studies, that of him a modern Johnson might well repeat—"Nullum quod tetegit non ornavit."

Yet none of his obituary notices recalled what will be his strongest claim to scientific fame, namely, that he was the first to demonstrate the presence of electrical currents in the brain. Indeed this work might well have remained buried were it not that Hans Berger in his classical paper of 1929, "Über das Elektrenkephalogramm des Menschen" (*Arch. Psychiat. Nervenkr.*, **87**, 527), writes:

> "Caton had already (1874) published experiments on the brains of dogs and apes in which bare unipolar electrodes were placed either on the surface of both hemispheres or one electrode on the cerebral cortex and the other on the surface of the skull. The currents were measured by a sensitive galvanometer. There were found distinct variations in current, which increased during sleep and with the onset of death strengthened, and after death became weaker and then completely disappeared. Caton could show that strong current variations resulted in brain from light shone into the eyes, and he speaks already of the conjecture that under the circumstances these cortical currents could be applied to localization within the cortex of the brain."

The references which Berger gives to Caton's work are three: *Brit. med. J.* (1875) ii, 278; *Zbl. Physiol.* (1890) **4**, Nr. 25, p. 785; Bechterew, V. M. (1902) Die Energie des lebenden Organismus. Wiesbaden; p. 102.

Caton's work which resulted in the earlier reference of 1875 appears to have been inspired by a communication of David Ferrier to the Royal Society in 1874 (*Proc. roy. Soc.*, **22**, 229) on the "Localization of Function in the Brain" in which he recorded the effects of ablation by cautery and of electrical stimulation of the cerebral cortex, in order to test "the theory of Hughlings Jackson, that localized and unilateral epilepsies are caused by irritation or 'discharging lesions' of the grey matter of the hemispheres in the region of the corpus striatum".

Ferrier mapped out those areas of the cerebral cortex which gave focal movement on faradic stimulation of the cortex and noted how these corresponded to the sites of paralysis which followed ablation of specific areas of the cortex. This paper was communicated to the Royal

Society by Dr. J. Burdon Sanderson, and was to be followed by two others, both entitled "Experiments on the Brain of Monkeys"—the first on April 29, 1875 (*Proc. roy. Soc.*, **23**, 409), and the second as the Croonian Lecture on May 13, 1875 (*Proc. roy. Soc.*, **23**, 431).

It was Ferrier's 1874 paper which led Caton to reflect that electrical currents might be detected in the brain even at rest. He applied successfully to the British Medical Association for a grant towards the expenses of his proposed investigation. By the spring of 1875 he had obtained interesting results, and a letter from Burdon Sanderson (undated but post-marked May 5, 1875) shows that Caton wrote to Burdon Sanderson who had sponsored Ferrier's papers, and who was then a Vice-President of the Royal Society, to enquire if it would be proper, since the grant for the work came from the B.M.A., to communicate his findings to the Royal Society. Burdon Sanderson answered:

> 49, Queen Anne Street, W.
>
> Dear Dr. Caton,
>
> I have not been so prompt as I should have liked to have been in reply to your letter.
>
> Unquestionably there can be no objection to you making a preliminary announcement to the Royal Society as to the results of your experiments. It would not, however, be a bad plan to give Mr. Hart for the Journal [Hart was editor of the *British Medical Journal*] a note at the time the paper is read to the effect following: 'On Thursday evening a note was communicated to the R.S. by Dr. Caton embodying certain very important results, etc., etc. Our readers are aware that Dr. Caton received a grant, etc., etc.'
>
> This would I think be gratifying to the Association and would be quite unobjectionable. It might come out the day after the reading of the paper.
>
> I am very glad that you have got such important results. Theoretically the subject is a very difficult one and wants many additional observation(s) to bring it into clearness.
>
> Yours truly,
>
> (Sgd.) J. B. SANDERSON.

On the flap of the envelope was a PS.—"Ferrier's new experiments are to be read next meeting, May 13."

Through the kindness of the Royal Society's librarian, Mr. I. Kaye, I have been able to search through the *Proceedings* and record books of the Society, but have failed to find any reference to Caton's having followed up his suggestion. Indeed, the first reference to his work in this field is recorded in the *British Medical Journal* (1875, ii, 278) where there appears a summary of a communication he made to the Annual Meeting of the B.M.A. in Edinburgh, in July 1875, when the Section of Physiology was meeting under the Presidency of Burdon Sanderson. This is the reference which Berger gives. It reads:

> "*The Electric Currents of the Brain.* By Richard Caton, M.D., Liverpool.—After a brief *résumé* of previous investigations, the author gave an account of his own experiments on the brains of the rabbit and the monkey. The following is a brief summary of the principal results. In every brain hitherto examined, the galvanometer has indicated the existence of electric currents. The external surface of the grey matter is usually positive in relation to the surface of a section through it. Feeble currents of varying direction pass through the multiplier when the electrodes are placed on two points of the external surface, or one electrode on the grey matter, and one on the surface of the skull. The electric currents of the grey matter appear to have a relation to its function. When any part of the grey matter is in a state of functional activity, its electric current usually exhibits negative variation. For example, on the areas shown by Dr. Ferrier to be related to rotation of the head and to mastication, negative variation of the current was observed to occur whenever those two acts respectively were performed. Impressions through the senses were found to influence the currents of certain areas; *e.g.*, the currents of that part of the rabbit's brain which Dr. Ferrier has shown to be related to movements of the eyelids, were found to be markedly influenced by stimulation of the opposite retina by light."

Caton published no further details of his experiments until 1887, when he read a paper entitled "Researches on Electrical Phenomena of Cerebral Grey Matter" to the Ninth International Medical Congress at Washington, D.C., U.S.A. (*IX Int. Congr. Med.*, 3, 246). This H. R. Viets uncovered in 1950.

In purpose and concept this paper shows how far ahead Caton was of his time. He was well acquainted with the electrical phenomena of peripheral nerves which DuBois-Reymond and Donders, amongst others, had investigated, but he could find only two references to electrical changes normally occurring in the brain. The first, published before his earlier communication, was DuBois-Reymond's paper on "The Electrical Current of the Frog's Brain"; the second was by James Dewar and J. G. M'Kendrick on "Experiments on the Effect of Light, in which a portion of the Brain was included in the circuit" (*Trans. roy. Soc. Edinb.*, 1876, **27**, 160). DuBois-Reymond's experiments recorded for the central nervous system what was well established in other tissues, viz. that an injured area is electrically negative to an intact area. The experiments of Dewar and M'Kendrick showed that light entering the eye causes electrical changes in the brain. They carried out experiments on both frog and pigeon and summarized the results of these:

"1. *The frog.*—On bisecting the head of a newly killed frog with a sharp pair of scissors, it is possible to obtain a longitudinal section of the brain, with the various parts in their natural position, then to carefully cut away the anterior and posterior portions of the brain, leaving only the middle portion in contact with the optic nerve. On placing this preparation between the clay points, so that the one touches the surface of the cornea, while the other is in contact with the brain substance, a strong deflection is obtained which is sensitive to light, and follows the course observed in the case of the frog.

"2. *The pigeon.*—The effect was also traced into the optic lobes of a living pigeon (deeply under chloroform), the head of which was securely held between the clay points of the electrodes. The optic lobes in the pigeon are easily exposed. The following were the effects of this observation:—*a*, When one pole was applied to the left optic lobe, and the other to the cornea of the right eye, a deflection was obtained which was sensitive to light; *b*, when the pole was removed from the right eye, and applied to the cornea of the left, a smaller deflection was obtained, also sensitive to light; and, *c*, when light was allowed to impinge on both eyes, while the one pole was in contact with either eye, and the other with the left optic lobe, the result was nearly double that produced by the impact of light on one eye alone, either right or left. These effects may be explained by the decussation of the optic nerves in the optic commissure."

Dewar and M'Kendrick demonstrated in effect that light shone into an eye gives rise to electrical currents which flow in brain tissue along recognizable paths.

Caton's earlier experiments and those reported in 1887 were of a different nature. They were designed to answer two questions: "1. Does the grey matter of the brain give evidence of electrical currents comparable with those of nerve fibre and muscle? (2) If such currents exist are they related to the functions of the brain, and will the study of such currents throw any light on those functions?" To answer these questions he used cats, rabbits and monkeys. He ingeniously applied small, light, non-polarizable electrodes with fine clay points to any region of the exposed brain which he sought to examine, and connected these electrodes by light insulated wires suspended from a support overhead to a reflecting galvanometer. The animal experimented on was tethered loosely to the centre of a table a yard square, and allowed to move about, eat and drink, at its pleasure. Many experiments failed from technical difficulties, but he was able to record currents from the surface of the brain which fluctuated, for example, with body movement or anæsthesia. But his observations of Ferrier's earlier described motor and sensory areas are of greatest interest and are best recorded in his own words:

"1. There is a region in the grey matter of the rabbit's brain, stimulation of which by the interrupted current causes rotation of the head to the opposite side. In the brain of the monkey there is also a corresponding centre. In several instances I found that by producing a sound, or by offering food on the one side of the animal experimented on, I could induce it to turn its head voluntarily to that side; when this movement was made, electrodes placed on the centre in question of the opposite hemisphere showed a fall in the current toward zero, in fact a negative variation; the movement of the needle exactly coincided with the movement of the animal's head to the opposite side. Probably the explanation is that the brain cells of the region were in a state of functional activity connected in some way with the head movement, and that a negative variation of the electric current occurred similar to that which is well known to occur in a nerve fibre when a reverse impulse traverses it.

"2. It is difficult to induce a rabbit or a monkey to perform any definite voluntary act and to repeat the action frequently enough for the basing upon it of a physiological inference. The act of mastication is more easily induced than any other. A rabbit will frequently eat a piece of fresh lettuce, and a monkey will usually eat a raisin or a piece of raw potato as soon as it is offered him. I experimented, therefore, frequently on that centre of the brain which when stimulated causes masticatory movements. In half the animals used, I found that when the non-polarizable electrodes were placed on this centre, negative variations occurred invariably when the animal masticated, the variations lasting as long as mastication and ceasing when mastication ceased. In some instances it was evident that the thought or expectation of food caused the movement of the needle. If I showed the monkey the raisin but did not give it, a slight negative variation in the current occurred. When the electrodes were applied to this region, I found that sensory impressions made on the mouth or face caused a similar movement of the needle; for example, the introduction of the handle of a scalpel into the mouth, pinching of lips or cheeks, or stimulation of skin of face by interrupted currents. It seemed from this experiment as though the centres for movement of jaw, for perception of sensory impressions from mouth and face, and for ideas of food derived through the eye, coincided or were closely adjacent to one another.

The area associated with these functions appeared to be small. I frequently had to search for it for some time. If the electrodes were not upon it but merely near it, no relation was observed between mastication and the movements of the galvanometer.

"3. Placing the non-polarizable electrodes on a given motor area, for example, Ferrier's region No. 1, related to the hind limb, I found that if I stimulated the limb with an interrupted electric current, negative variations frequently, though not always, occurred. Stimulation of other parts of the body had no such effect. This experiment seemed to indicate that the centres for the production of muscular movement and for sensory perception in the skin coincided for the limb in question.

"4. Not unfrequently after continous exposure of a hemisphere of the brain, spasms occurred in one or both limbs on the opposite side. If I placed the non-polarizable electrode on the brain area corresponding to the movement, I usually found that a strong negative variation coincided with each spasm. This was seen in numerous experiments, though not invariably.

"5. I found no part of the brain the electrical currents of which were influenced by stimulation by odors or by sound.

"6. I tried the effect of alternate intervals of light and darkness on seven rabbits and four monkeys, placing the electrodes on the region (13) stimulation of which causes movement of eyes. In three rabbits and two monkeys I found that light caused negative variation almost invariably. In those five experiments in which I was successful the relation between the intervals of light and darkness and the movements of the galvanometer needle was quite beyond question. If I partially shaded the animal's eye from the light, the effect on the electric current was diminished. The exact way in which the light produced its effect is not so easy to determine. It may have excited the visual centre especially, or it may have acted as a general excitant to the whole brain, or the result may possibly have been due to the heat radiated from the flame acting on the electrodes; I think one of the first two theories is more probable than the third."

In brief, Caton showed that electrodes applied to the brain manifested electrical currents which increased during the suspension of functional activity. These electrical currents were modified by activity of the cortex being almost invariably diminished when the cortical area examined was functionally active. He regarded the electrical change demonstrated in Ferrier's areas which accompanied the motor or sensory activity associated by Ferrier with these areas, as further presumptive proof of cerebral localization. There is in these observations clear anticipation of the later observations of Berger, Adrian, Penfield and others.

It is difficult to evaluate Caton's work with precision in the absence of exact data relating to the sensitivity of his galvanometer (he describes it as Sir William Thomson's reflecting galvanometer) and of the temporal shape of the observed changes in current. Emeritus Professor H. A. Ormerod, his son-in-law, tells me that all Caton's manuscripts, records and instruments relating to his physiological work were destroyed several years ago. There is the natural danger of reading into the experiments more than the technical shortcomings would justify, but none can doubt that they were most advanced in conception for their time, especially the attempt to observe these electrical changes in the cortex of an animal—mobile and active. Some have ascribed the findings to injury potentials similar to those described by DuBois-Reymond in peripheral nerves, but this cannot be the explanation of the influence on the magnitude of deflection of such factors as functional activity of the cortex, and anæsthesia, and the act of dying. There are, however, certain difficulties to explain. For example, it is difficult to see how a motor discharge and an afferent stimulus could produce deflections of the same polarity: if the cortical surface on discharge becomes negative in relation to a distant point, it should become positive when an afferent impulse is travelling upwards, as indeed was found by Adrian and others. Further it seems unlikely that the mere *sight* of food would be sufficient to produce currents from the sensorimotor area for the face. The same criticism may be applied to the localization of the visual centre in area (13), the area controlling the movements of the eyes.

Not the least interesting offshoot of this work is that Caton became involved in a claim for priority. This is worth relating because it adds to our knowledge of the development of electrocorticography, and illustrates vividly a somewhat discreditable feature of the science of that period. On November 8, 1890, there appeared in the *Zentralblatt für Physiologie* (**4**, 473), a paper by Dr. A. Beck, Assistant in Physiology in the University of Cracow, on "The ascertainment of the localization of the functions of the brain and spinal cord by the electrical manifestations." He sought "to prove in a direct way that by stimulation of a centripetal nerve the centres of this nerve come into action". He made two sets of experiments, the first on the spinal cord of frogs; the second, and these are relevant, examined the currents of the cerebral cortex in warm blooded animals. In dogs and rabbits, he sought for the cortical centres of vision, touch, and hearing. He and Caton used similar electrodes (those of DuBois-Reymond) but his galvanometer was Hermann's, compensated by rheochord to measure power and observe direction of current flow. He describes his experiments thus and I would stress especially that part of his description which appears to anticipate Berger's α-rhythm:

"I denuded one hemisphere of the surrounding soft parts and bones, being careful not to damage the dura mater before the total denuding of the hemisphere. After this the dura mater was carefully split and turned over on the edges of the bone which were blunted and smoothed as much as possible. On two points of the cerebral cortex I placed the non-polarizing electrodes. Already at the first experiment I noticed—and this was constantly repeated—that the range of movement after the connecting of the electrodes to any two points of the cerebral cortex of the hemispheres was not a steady one; the fluctuations were not synchronous with the rhythm of respiration nor with the pulse, nor were they dependent on any movements of the animal since they appeared also in curarized dogs. Therefore, neither the pulsations of the brain nor changes in the cortical vessels nor voluntary movements can be considered as the cause of these oscillations. They were entirely independent and stopped during stimulation of centripetal nerves as well as under a deep chloroform narcosis.

"I feel, therefore, entitled to consider these oscillations as the expression of continuous changes which take place in the condition of the action of the cortical centres. This activity can be suppressed by the stimulation of a certain group of centripetal nerves and was entirely stopped by the narcosis.

"On stimulation of the eye by magnesium light an electro-negative tension arose in the occipital lobe of the opposite hemisphere of the brain. A strict limitation of the area of vision was found in the dog while in the rabbit they were scattered all over the posterior part of the hemisphere, which fact agrees with the results of the experiments of Munk.

"The manifestations were not so distinct on stimulating the sense of hearing by sound as by stimulation of the eye. The reason for this may have been that the connexion of the electrodes immediately to the lower surface of the temporal lobe caused great difficulty. However, I found on stimulating the nerve of the skin that the current derived from the corresponding part of the cerebral cortex showed distinct changes which gave some indication as to the origin as to the condition of action."

This was followed in the next issue of the *Zentralblatt* (**4**, 537) by a letter which indicates a practice not uncommon, especially on the Continent in those days, namely, attempting to secure priority by depositing a letter embodying preliminary findings, usually incomplete and unsubstantiated, on any scientific problem. This letter was from the Professor of Physiology in the University of Vienna, the celebrated Ernst Fleischl von Marxov. It read:

"The contents of the original paper of Dr. Beck prompts me to publish a sealed letter which was deposited with the Imperial Academy of Sciences in Nov. 1883, and which was opened and read at the last meeting of the class for mathematics and natural science by my request.

"The letter which was so opened had literally, without any addition or omission, the following text:

'Vienna, 6 November, 1883.

In the course of this year, I have made a series of experiments on different animals, the results of which appear to me important enough to secure for myself priority concerning these findings by the deposition of this letter with the Imperial Academy.

'If one connects two symmetrically lined points of the surface of the cerebral hemispheres to a sensitive galvanometer by non-polarizing electrodes one will not find any or only a very little movement. If one stimulates an organ of sense the central projection of which is one of those points connected with the galvanometer, one will get a movement in a certain direction; if one stimulates the corresponding organ of sense of the other side, one gets a movement in the opposite direction. The experiment for instance succeeds very beautifully by connecting the points which have been defined as the centre for the perception of sight by Munk on both sides and by alternately exposing the one or the other eye to light.

'If one leaves the electrodes on the places just mentioned and stimulates the animal by vapour of ammonia which one allows to act on nasal mucosa, or if one pinches the extremity of an animal, or one burns it with a hot needle, one gets none or extremely weak movements, obviously caused by spread of current. One is, however, able easily to find those points of the cerebral cortex by this method where strong disturbances of the electric equilibrium are caused by the stimulation in question so that one can use this fact as a method to find those parts of the cerebral cortex where certain sensory stimuli are transmitted to our consciousness.

'If you chloroform an animal in which these experiments have succeeded and repeat the experiments during the narcosis of the animal, one does not get even the trace of a reaction in the galvanometer. If one allows the animal to wake up and repeats the experiments again one gets positive results again. From this follows, firstly, a confirmation of the conclusions drawn from the experiments generally, and secondly, that the narcosis by chloroform (and ether) is really due to temporary paralysis of the cerebral cortex, and not as some people believe to an interruption of the memory.

'These experiments succeeded not only by direct connexion with the denuded cerebral cortex, but also by connexion with the corresponding points of the dura mater, even with the bones of the skull deprived of the periosteum. One has to be careful in these experiments that the cortex does not cool, for this also causes paralysis. It will perhaps even be possible to perceive the currents arising by different psychic acts of one's own brain by connecting the skin of the head.' "

Professor Marxov's communication continues:

"On the contents of this seven-year-old letter, priority is claimed.

"Into the differences in a few details of our observations I am not disposed to enter because I will presently find an occasion for critical comment on the communication of Dr. Beck, or for a detailed comparison of the worth of the two observations.

"But I must briefly point out one circumstance, that in the results of my investigations the answer is contained to the question which hitherto has remained unsolved, i.e. whether narcosis brings about an interruption of the faculty of memory or a temporary annihilation of perception? Finally, it is perhaps superfluous to stress that in spite of the authorization of my claim to priority, the merit of Dr. Beck's investigations remains undisturbed, for he could have had no knowledge even of the existence, let alone of the content of my sealed communication to the Imperial Academy of Sciences in Vienna."

This letter from Professor Marxov stimulated a pertinent reply from Dr. Beck (*Zbl. Physiol.*, 1890, **4**, 572. He wrote:

> "Nature held and still holds innumerable problems in its secret. But it is no difference to science if the solution of one of these puzzles remains under the seal of nature itself or under that of the Imperial Academy of Sciences in Vienna. The priority of the discovery, therefore, is due to the one in my opinion who has broken the seal of nature of a secret without bringing it under another one.
>
> "Besides the struggle for the priority in this case appears to me at least unnecessary. This is not a discovery but the application of an already known method to the solution of new questions."

Beck then tells how he had been led to his research by the examination question set for a prize which he gained. The question was: "It has to be stated if one is able to demonstrate the condition of action of the nerve centres by so-called negative movement, and in the case of positive results to demonstrate by negative movement: the localization of the reflex centres for the lower extremities in the spinal cord, the centres of vision of the cerebral cortex and the automatic centres in the medulla oblongata."

Beck then refers to earlier research in the electrophysiology of the nervous system by DuBois-Reymond, Hermann and Sieczenow, but gracefully concludes: "Lastly let me say how pleased I am that my results have been so rapidly confirmed by so admirable a scholar as Professor Marxov."

Following this Caton wrote to the editor a letter published on March 14, 1891 (*Zbl. Physiol.*, **4**, 785) in which he refers to the priority claims of Marxov and Beck for the discovery of electrical currents in the cerebral cortex. He continues:

> "In 1874, the B.M.A. awarded me a grant to investigate the electrical changes in the cerebral grey matter. In 1875, I made a report to the physiological Section of the B.M.A.

Then follows the full text given above of the report of the paper in the *British Medical Journal* (1875, ii, 278). After referring to his Washington paper in 1887, he concludes:

> "It is not my purpose to belittle the fame of the learned physiologists, still I did make these discoveries myself, and as I mentioned above, have published them, so I think it will be admitted that I was already the earlier discoverer."

This closed one episode in the life of a man who held that physiology was the basis of sound medical practice, and applied the scientific method to his problems, at a time when empiricism held undue sway. He sought a rational basis for his therapy. For example, in rheumatic fever he asked himself the pertinent question—"Joints recover: why does the endocardium fail to do so?" And the answer came, "The rheumatic joint rests, but not the rheumatic heart", and so he instituted complete rest for months. He contributed to the explanation of the effects of counter-irritation by showing that stimulation of the skin of the chest, thermal, electrical, mechanical or chemical, produced changes in the calibre of the arterioles of the lung, also that similar stimulation modified the electrical potential of the intestinal wall, as shown by the galvanometer. These observations led him to try the effect of small blisters over the third, fourth, fifth and sixth intercostal spaces in acute rheumatic heart disease, long known as Caton's blisters. He read widely and was quick to apply the knowledge so gained. Within a few months of Pierre Marie's recording that acromegaly is associated with pituitary tumour, he had induced F. T. Paul to operate on a patient afflicted with the disease.

But above all Caton stressed the whole man, and it was his yearning for human betterment that motivated him in all his work—whether in treating a patient, in campaigning for slum clearance, for secondary and University education, for a great new Cathedral, or for the many other causes dear to him.

I recall but one short chapter of his life and contribution to medicine here, not from a feeling of parochial pride, though I am indeed proud to belong to a Medical School of which he was one of the most luminous figures, but also to pay a somewhat belated tribute to one who made a notable and perspicacious contribution to physiology, and who saw what was hidden from many of his contemporaries, the signal part which physiology was destined to play in the development of the science of medicine.